

XV.—Notes on the Review of G. R. Gray's "Catalogue of the Genera and Subgenera of Birds" in the December Number of the 'Annals.' By G. R. GRAY, F.L.S. &c.

IN a recent notice of my "Catalogue of the Genera and Subgenera of Birds," published in the 'Annals and Magazine of Natural History' for December 1855, a number of objections are stated to some of the minor details of that work, which might lead the reader to suppose that its author had been actuated rather by caprice than by principle in the matters referred to, and would therefore give an erroneous impression of the nature of the work, and of its utility to the ornithological student. With the view of guarding the reader from such an impression, I am desirous of putting him in possession of my reasons for adhering to the principles which, after long and anxious consideration, I had deliberately adopted, and which twenty years' unremitting attention to the subject has only served to strengthen and confirm.

Let me observe, in the first place, that no edition of my work was published in 1844, although that date is given to a previous edition of it by the reviewer in several places. As however he mentions this previous edition as containing "upwards of 1100 distinct types," it is probable that the original edition of 1840, in which that number of generic types is to be found, is the one referred to. The only other edition (the second) previous to the one now noticed, was published in 1841.

Passing over the observations on the multiplication of generic names by the same authors for the same generic types, and on barbarously compounded generic names, with which I have nothing to do but to record them, I come to the first objection taken by the reviewer, in regard to misspelt names. "There seems," he says, "to be no reason whatever why such an error should be retained *in perpetuum*," and adds, that "Mr. Gray appears to hold, that right or wrong we are bound to adopt the spelling originally given by the proposer of the genus, and to allow of no corrections or emendations even of faults due to typographical errors only." This is a strong charge, to which I distinctly plead "Not Guilty." I certainly hold no such opinion, and I am not aware of any statement of mine by which the charge can be supported. It is true that in a work destined to give, in a concise form, the history of each division, I think myself bound to record even the variations in spelling that may have been used by different authors, or by the same author at different times; and when the reviewer asks "what benefit can we derive" from such a record, I answer without hesitation that, for want of this information, naturalists frequently lose much time, and sometimes unavailingly, in their search in indexes and elsewhere for particular names, because the spelling has been varied from that with which they are familiar. In some cases too the etymology may be doubtful, and the proper mode of spelling not easily decided. The introduction of these variations is consequently in my opinion a useful addition to such a work; and it is moreover justified by the

example of other authors, both in this country and abroad, who have not thought it desirable to leave them "unnoticed and forgotten."

The reviewer next expresses his "fear that confusion is likely to be caused by the introduction of the French names which Mr. Gray has permitted in some parts of his list." If the objection had been that these names have been introduced too rarely, it would, as it seems to me, have been better founded. No harm can result from their insertion, except the unpleasantness arising from the indication that, in too many instances, those who have done nothing more than apply a Latin name to a division already clearly established under a French one, have thus cheaply obtained the credit of having established that division for themselves. To avoid the necessity of appealing to recent cases, let us pass for a moment from genera to species, and call to mind the natural indignation which has been universally felt and expressed at the wholesale appropriation by Gmelin of hundreds of species of birds established by Latham under English names, but which one of the most ignorant of compilers conveyed to himself simply by converting Latham's English into Latin. For this reason, on the plain principle of *suum cuique*, it will be my endeavour to increase rather than to curtail the citations of such names, the Latinization of which, in many cases, requires merely the slightest alteration in the termination to render them much more euphonious than the Greek compounds, which it has been proposed to substitute in their places. Thus the *Picazuros** of M. Lesson have been latinized by M. O. Des Murs under the generic name of *Picazurus*; and I think no one will deny that *Picazurus gymnophthalmus* would be at once a better-sounding denomination than *Crossophthalmus gymnophthalmus*, and more just to the original author of the division.

A modern author of some note was considered to have overcharged a branch of Ornithology "with new and useless denominations," because he gave Greek compounds to those divisions which had previously received French names; while I am accused of causing confusion by simply recording the existence of these previous names.

The next point on which the reviewer thinks the principles which I have adopted "do not work well," has reference to the question "what edition of the 'Systema Naturæ' we ought to begin with,"—a question which he says "has been already discussed in a previous review of a former edition of Mr. Gray's book in this Magazine;" and a note at the bottom of the page refers us to "Mr. Strickland's article in the 'Annals and Magazine' for 1851." The date, however, is widely incorrect, Mr. Strickland's article having been published in January 1842. In that paper the author, after some mistaken remarks on Mœhring, thinks "a strong case" has been "made out for establishing a statute of limitations." "Let naturalists," he continues, "agree once for all, to draw an absolute line at the date of 1760, when the elaborate standard work of Brisson

* This word is wrongly printed in the Catalogue as *Picazores*, an error copied by the reviewer.

appeared, and when the 'binomial method' was first dawning on the mind of the great Linnæus, and let them admit no genera on the authority of any prior author, nor even of the earlier works of Linnæus himself." To this purely arbitrary decision I can find no reason whatever for subscribing. In my work it is justly stated, "The synonymy commences with the edition of Linnæus's 'Systema Naturæ' published in 1735*," that is to say, with the first edition of that immortal work; and I have yet to learn in what respect this principle does not "work well." The question has nothing to do with the "binomial method," which has reference only to species; and Linnæus himself discriminates between the earlier formation of genera, which were well circumscribed and accurately named long before the complete circumscription and limitation of species by the use of trivial names. For this reason, any "statute of limitations" in regard to genera that should stop short of 1735, would rest on no intelligible principle, and could not therefore command a general assent. I will only observe further, that were the date of 1760, as proposed by Mr. Strickland, to be taken as the "absolute line" of the "statute of limitations," it would exclude the great and universally quoted edition of the 'Systema Naturæ' (the tenth) published in 1758, in which the binominal system was complete in regard to Birds; and the binominal system was not even then merely "dawning on the mind of the great Linnæus," but had been fully carried out through the whole vegetable kingdom in the edition of the 'Species Plantarum' published in 1753.

The edition of the 'Systema Naturæ' published in 1735 being then taken as the starting-point, from which the great author of a uniform system proceeded in the establishment of genera, it is objected to me that I seem "to give that and the other earlier editions an occasional preference over the subsequent and more perfect publications." The fact is, that all the editions are referred to, for the purpose of showing, in conformity with the entire plan of my work, when the genus was first proposed and established by Linnæus. And here, as elsewhere, I offer to every student the means of tracing out the facts necessary to complete the history of each division, being quite aware of the natural divergence of minds on all questions of opinion, and leaving it open to all to form their own opinions in conformity with those principles which appear most satisfactory to themselves. My aim is solely to produce a record of facts as complete as possible, and I make no pretensions to the vain attempt of producing uniformity of opinion.

The reviewer goes on to object that I take "it for granted that the first species on the list of each of these editions was intended to be the type of the genus,—a point which admits of much argument." It is with the view of saving "much argument," which would assuredly be the result of any other system, that I have laid it down as a principle *for my own guidance*, that where no other species is stated

* Systema Naturæ, sive Regna tria Naturæ systematice proposita per Classes, Ordines, GENERA et Species. Lugd. Bat. 1735.

by the author as typical, it is the safest, best, and only certain rule, to regard the species first enumerated as the type. This is a subject to which I have given much thought: some rule was found to be absolutely necessary; it was in the highest degree desirable that the rule should be uniform; and the principle adopted was the only one, which after long and careful deliberation appeared to me to fulfil the required conditions. Others may, if they think fit, and as some modern authors have done, take the tenth or the twentieth species in the list as the type of a Linnæan genus, and may give plausible reasons for so doing; but all must admit that such a course is one leading to interminable argument, and leaves the door open to much individual caprice.

The reviewer proceeds to give instances in which he considers me to be wrong. "*Chenalopez*," he says, "(a term *always* hitherto appropriated to *Anas Ægyptiaca*,) is proposed to be used for the *Alca impennis*, as having been so applied by Mœhring in 1752." Now it so happens that Vieillot adopted this generic name from Mœhring in his "subsequent and more perfect publication" of his "Analyse" in 1818; while Stephens did not employ the same word for *Anas Ægyptiaca* until 1824. I think the reviewer will *now* admit that *Chenalopez* has not *always* been appropriated in the manner stated by him. And let me here observe in behalf of this unfortunate author (Mœhring), whose work ('*Genera Avium*') I have been charged with disturbing from the "dusty shelves" on which it had lain "forgotten for a century," that long before I could have written or published a single word, his work had been considered worthy of quotation by Brisson, Illiger, Cuvier, Vieillot, Lesson and others, through whose writings I first became acquainted with his merits.

Again, the reviewer says, "The type of the genus *Tanagra* is altered, because the *T. episcopus* (*always* hitherto considered as such) does not stand first in Linnæus's list." Here again the reviewer is in error. *Tanagra* was established by Linnæus in 1766, and the first species in his list is *T. jacapa*. In 1805 Desmarest considers *T. tatao* (= *Aglaia*) as the type of *Tanagra*; in 1811 Illiger, taking the first-named species in Linnæus's list, recurs to *T. jacapa*; in 1816 Vieillot gives *T. cayanensis* (= *Iliolopha*); in 1817 Cuvier adopts *T. violacea* (= *Euphonia*); in 1820 Temminck takes *Lanius leverianus* (= *Cissopis*); while it is not until 1827 that Swainson proposes *T. episcopus* as the type of the genus *Tanagra*. But, even were it possible to set aside all the previously proposed types of this genus, there still remains a fatal objection against this last-named appropriation of the name, if "the stern law of priority" is to have any weight, inasmuch as M. Boie had in the previous year proposed the name of *Thraupis* for a species which must be arranged along with *T. episcopus*; and consequently, were the views of the reviewer to be critically carried out, the name of *Tanagra* would be erased from the nomenclature of the Tanagers altogether. To this conclusion I am not prepared to follow him; any more than I can admit, after the above recapitulation of facts, the correctness of his state-

ment that *T. episcopus* has always hitherto been appropriated as the type of the genus *Tanagra*.

If I have "not ventured to carry out these rules [that is to say, the recognition of the first-named species of a group as its type, when no other is indicated as such] throughout to their legitimate result," I have at least shown in the two instances especially cited, *Strix* and *Falco*, how the matter stands in relation to them. Throughout all the editions of Linnæus from 1735 to 1766, *Strix Bubo* is uniformly placed at the head of his genus *Strix*; while the modern innovation of considering *Strix flammea* as the type was not legitimately adopted until 1809 by Savigny; and so much has the propriety of this determination been doubted, that no fewer than four ornithologists have since proposed as many different names for the division of which *Strix flammea* forms the type. Had I therefore "ventured" to meddle with this, which I cannot but consider as an unfortunate appropriation, I could not have been charged with an infringement of the maxim "*quieta non movere*." So with regard to *Falco*, it will be seen by the quotations in my work itself in what manner Linnæus altered his opinion as to the species to be placed first, as follows:—in the editions of 1735, 1744 and 1758, *Falco chrysaetos* (= *Aquila*); in those of 1748 and 1756, *Vultur perenopterus* (= *Neophron*); and finally in 1766 *Falco coronatus* (= *Spizaëtus*).

But I will not pursue this subject farther. In my work it will be found that I have endeavoured to give as complete a view as possible of the facts, by referring to these and similar changes, in order to assist the student in the application of his own particular views to the facts of each particular case. I feel abundantly satisfied that the adoption of the "statute of limitations," as proposed, would have led to the alteration of many more names than I have "ventured" to change, by adopting as my guide the first species of each genus, as it stood when first established. No "statute of limitations," nor any other rule but that of mere caprice, would sanction many of the types adopted for the older genera by modern authors, whose great fault it has been that they have disregarded the labours of their predecessors, and thus involved themselves in those numerous uncalled-for alterations and repetitions against which the reviewer so justly declaims.

I pass over the remarks on the subject of names closely resembling each other, as it is probable that no two persons would ever agree on the exact degree of permissible similarity in sound or spelling, and it is certain that the natives of different countries would entertain different opinions on the subject; and come next to the reviewer's expression of "regret that Mr. Gray had not thought fit to adopt the very simple rule given in the British Association Committee's Report for the formation of the names of the families and subfamilies in *idæ* and *inæ*." On this point (which is quite secondary to my main object of "Genera") I have to observe, that the rules which I have adopted were collected, as the best that had been proposed by my predecessors, and those which appeared to me to combine most completely the principles of fairness and justice towards

others, prior to the publication of the Committee's Report. I saw in it no inducement to change them, and I have not found that the most competent judges have adopted the changes of nomenclature therein recommended. For instance, I am blamed for using the word *Coraciadæ* instead of *Coraciidæ*; yet I perceive that the President of the Linnæan Society, in the title of a very valuable memoir in the last published part of the "Transactions" of that learned body, does not hesitate to employ the similar term *Leucosiadæ* in preference to *Leucosiidæ*, as directed in the Committee's Report.

The reviewer's suggestion of "a Catalogue of the unabbreviated names of the authors of the different genera, and of the chief works in which they have published them," is one that has not escaped my attention. I have in my possession an extensive list of authors, accompanied with references to their works; but it is not my intention to publish it at present, although I may find occasion to do so hereafter.

In relation to the names of genera proposed by Dr. Schiff (to which the reviewer might have added the names of Dr. Reichenbach and others), I held it to be my duty to give all the generic and sub-generic names that came within my knowledge, whether accompanied by the statement of the typical species or not. I have fortunately been enabled in most cases (with the exception of the names of Rafinesque) to supply this deficiency; and I hope that I may thus have been the means of preventing, to a certain extent, the multiplication of names for the same divisions, although I do not attempt, as it would be useless, to set limits to the subdivision of genera. The addition of the name of the publisher, as well as of the author, would have involved the total reconstruction of my book on a different plan.

This article is longer than I had intended, but I must be permitted to end it with the words of a well-known ornithologist:—"We have chosen our path:—not having fallen into it by blind chance or wayward prejudice; but having selected it from all that lay before us, with free and deliberate preference. And in full confidence, as far at least as human reason and foresight can inspire us with confidence, of having chosen the right way, we shall steadily pursue it."

BIBLIOGRAPHICAL NOTICES.

A Popular History of Palms and their Allies. By BERTHOLD SEEMANN, Ph.D. &c. London: Reeve. 1856.

IN introducing his subject to the reader Dr. Seemann states, that his attention was first directed to the family of Palms through inquiries set on foot in his school days, in connexion with the conversion of his pedagogue's cane into succedanea for cigars. We cannot lay claim to the possession of so inquiring a spirit in our youth, or at all events it did not take that direction. The associations connected with the name of palm-trees in our minds, and we fancy in those of most persons, are of a more elevated and less practical nature. To